8

MIGRATION AS A CULTURE TRANSPLANT

Neoclassical and Institutional Channels

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8.1 Introduction

Do migrants make the economies they move to a lot like the ones they left? At least in some cases, the answer must be an emphatic yes. When Acemoglu, Johnson, and Robinson (2001) refer to the U.S., Canada, Australia, and New Zealand as "Neo-Europes," we intuitively know the meaning of that term: after 1500, migrants from Europe moved in their millions to these nations, and imposed European-style institutions upon them with violent force. And so it is little surprise that the economies of these Neo-Europes are at least as productive today as Western European economies—they're close cousins, after all.

An important question for social scientists is whether such migration-driven "culture transplants" have occurred, on average and in the long run, beyond this handful of Neo-Europes. And it's particularly important to know whether such culture transplants are likely to occur in the 21st century as well: perhaps culture transplants happened in the past, but perhaps now migrants rapidly and quickly assimilate to their new homes, having negligible effects on the institutions of the countries they move to. Alternatively, maybe migrants and their descendants import new attitudes, but perhaps government institutions and national productivity are overwhelmingly unresponsive to the views and behaviors of these new citizens, so perhaps there's no important link from changes in the views of voters to changes in institutional outcomes.

Moral consequentialists—utilitarians in particular—should be deeply interested in whether migration to the world's rich countries is likely to either improve or worsen institutional quality in the nations that migrants move to. That's because the vast majority of the world's innovation, the world's research and development (R&D), occurs in just seven countries: Japan, China, South Korea, the U.S., Germany, the U.K., and France. These nations, which I've called the I-7, produce pharmaceutical research, electronics innovation, and engineering marvels that, eventually, improve lives in nearly every nation on Earth (Jones, 2022). These nations are the research and development labs for the entire planet, and since a nation's level of R&D is highly sensitive to the quality of that nation's govern-ment institutions, any decline in institutional quality in the I-7 places future improvements in well-being for the global poor at risk.

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The traditional view of the cruel tradeoffs of migration is captured by the works of Borjas (1994, 2003) and Clemens (2011). Clemens correctly notes that the benefits of migration to migrants from poor countries are absolutely massive—his "trillion dollar bills on the sidewalk." And Borjas notes that lower-skilled previous residents of the nation—so-called "natives"–likely suffer small wage declines due to competition from immigrants. The conventional way to evaluate the utilitarian or consequentialist merits of freer migration to rich countries is then to compare the massive benefits to the migrants against the apparently smaller costs to the so-called native-born. It's a comparison of a benefit going to people currently living in poor countries against a cost to people currently living in rich countries.

But once we turn our attention to the I-7's crucial role in globally shared innovation, we realize that there may be a new, far crueler tradeoff: the benefits of migration to people moving from poor countries to rich countries versus the possible long-run impact of that migration on global innovation and on productivity in the world's poorest countries. If migration to rich countries hurts institutional quality in those rich countries, then the migrating poor may pick up their trillion-dollar bills today at the expense of generations of future poor people around the world, who may thereby lose the equivalent of many more trillions of dollars worth of future live-improving innovations. Whether migration to rich countries shapes government quality and economic innovation in those rich countries is thus a question of first-order importance for anyone interested in the overall effects of migration on human well-being. The cruel tradeoffs of migration policy may well be global in scope and may extend across many generations.

Two vast empirical literatures have proven indispensable in shaping answers to the question of whether migrants—and importantly, their descendants—import cultural norms that shape government institutions and productivity. The first is known, regrettably, as the "epidemiological approach" to cultural transmission, and the method is simple. Investigators draw upon modern surveys like the World Values Survey (WVS), the General Social Survey (GSS), and the European Values Study, and check to see if, for example, today's second-generation Italian-Americans hold social and policy views a lot like people in Italy today (*inter alia*, e.g., Giavazzi et al., 2019; Borrell-Porta et al., 2017).

The research is valuable because it easily and repeatedly demonstrates that second-generation migrants whose ancestors came from different countries really do hold different attitudes, on average, depending on where their ancestors came from. In some cases, researchers look beyond the second-generation to the third- and fourth-generation—the grandchildren and great-grandchildren of migrants, and they generally find substantial persistence, even with their noisy proxies for ancestry. This stylized fact makes belief in full, rapid assimilation untenable. To be sure, the relationship is stronger for some views than others, and for some survey questions, full assimilation really does appear to happen. But for savings behaviors, views on the role of government, and generalized trust, this "epidemiological approach," which I'll instead call the attitude migration approach, finds strong evidence of multi-generational persistence among migrants. Since, particularly in democracies, governments tend to move in the direction of voter attitudes (Caplan, 2007), this offers a plausible mechanism for the theory that the descendants of migrants would make the countries they go to at least somewhat like the ones they left.

The second relevant scholarly literature is the Deep Roots literature, sometimes known as the persistence literature. The portion of this literature relevant to our question—the migration-adjusted Deep Roots literature—grew out of a paper by Putterman and Weil (2010) that focuses on the role of migration in explaining modern differences in productivity across

countries. They showed that on average, regions of the Old World where there was long experience living under organized states or long experience with settled agriculture tend to be more prosperous today, and tend to have better institutions; the past was prologue.

And that relationship held strongly when they included the New World as well—but only when they adjusted for migration. In other words, it only held strongly when Putterman and Weil took account of the fact that many people moved, both voluntarily and all too often involuntarily, from places with differing lengths of experience with states and agriculture. When people—particularly people from Western Europe and China—moved from places with longer experiences with statehood and settled agriculture to places where the residents had much less such experience, these migrants' new homelands ultimately became a lot like the places they'd left. These migrants did not fully assimilate to the local norms. Many scholars have built on the Putterman and Weil migration-adjustment approach, and the evidence thus far is compelling: if one wants to use data from the distant past to predict a nation's prosperity today, one should always "adjust for migration."

The remainder of this paper draws out the methods and messages of these two literatures in greater detail, drawing in part on (and occasionally drawing verbatim from) my recent book with Stanford University Press, *The Culture Transplant* (2022). Section 2 reviews the attitude migration literature, which finds that many important attitudes appear to migrate and are transmitted in large part to at least the second generation. Those attitudes include views on the role of government, the role of women in society, and views toward generalized trust. A smaller literature looks at actual behaviors, in particular, whether savings behaviors are partly transmitted to the children and even grandchildren of migrants. Section 2 concludes by reviewing the political economy evidence that democracies are responsive to changes in the electorate, so new people, on average, create new policies.

Section 3 reviews the migration-adjusted Deep Roots literature, looking in particular at three well-studied measures: ancestral experience living under organized states (S), settled agriculture (A), and ancestral experience in the year 1500 using the world's frontier technology (T). Together, I informally denote these as a nation's SAT score.

Section 4 applies these literatures to the important test case of Southeast Asia, a region that has been heavily shaped by overwhelmingly peaceful migration from people from China over the last few centuries. Singapore stands out as a strong illustration of cultural transplant theory, but Malaysia, Thailand, Indonesia, and the Philippines all have Chinese ethnic minorities who continue to pass along, to some degree, the deep roots of Chinese culture. The Southeast Asian experience is particularly important for illustrating how peaceful, voluntary migration can shape institutional quality.

Section 5 addresses some of the criticisms of cultural transplant theory that have arisen in recent years, and Section 6 concludes.

8.2 Attitude Migration

A continuing stream of empirical papers shows that, as Alesina et al. (2015) put it:

> Cultural values are relatively slow to evolve, as a vast literature on the behavior of immigrants to other countries, mainly the United States, shows.

Much of the attitude migration literature—but as we'll see, by no means all—checks to see whether second-generation immigrants hold views a lot like those in their parent's

country of birth. As a reminder, a second-generation immigrant is a child of immigrants, someone whose parents were born in a different country than she was. Why study the second-generation? In part, because first-generation immigrants are largely self-selected, they're quite likely to be quite different from the average citizen back in their country of origin. A first-generation immigrant to France may have chosen to go to France because she knew she had a set of cultural, linguistic, or economic preferences and skills that made France a good fit for her; she may have chosen France precisely because she thought she could assimilate quickly. Second-generation immigrants, by contrast, didn't get to decide where they were born, so national self-selection can't be driving the results.

8.2.1 *Neoclassical Examples*

The earliest paper of which I am aware to use the methodological approach discussed here doesn't look at attitudes at all; it looks at concrete behaviors. Fernández (2011) notes that an influential paper by Antecol (2000) used male and female labor force participation…

> in the country of ancestry to examine whether culture plays a role…in the inter-ethnic gender gap in labor force participation rates in the United States.

Indeed, Antecol found evidence that home-country gender gaps in labor force participation were partly transplanted to the U.S. in the second generation. This paper was the start of the "vast literature" that Alesina et al. mentioned above.

Fuchs-Schündeln et al. (2020) use country-of-origin data from the WVS to predict the savings behaviors of second-generation migrants to Germany; they find that a one standard deviation increase across countries in whether children should be taught the value of thrift predicts a rise in the second-generation migrant's savings rate by between 0.8 and 1.6 percentage points. A one standard deviation increase is, informally, comparable to the average difference you'd find between any two countries in the researcher's sample. The range of estimates arises from differences in the statistical specification—in what the researcher "controls for" or attempts to hold constant when making comparisons across people whose parents came from different countries.[1] [Costa-Font et al. (2018) find broadly similar results for migrants to the U.K., extending results to the third generation.]

The numbers from the Fuchs-Schündeln paper may sound small except for the fact that the mean savings rate in their sample is 7.7%. That means they're reporting between a 10% and a 21% increase in the savings rate. For comparison, William Gale of Brookings long ago reported that shifting to a flat-rate income tax would plausibly raise the U.S. savings rate by half a percent to 1% of GDP, a 10% to 20% rise in total savings (Gale, 1996). Thus, a one-standard-deviation rise in thrift in a second-generation migrant's nation of origin is comparable, per person, to the plausible effect of one of the most widely discussed pro-savings tax reforms. The narrow neoclassical effects of migration policy on national productivity appear too big to ignore.

8.2.2 *The Migration of Familism*

Alesina et al. (2015) investigate a more conventional form of attitude migration: not attitudes toward frugality but attitudes toward the proper role of government. They only look at second-generation migrants to the U.S., and find, as so often, that attitudes partly

migrate from the country of origin to the U.S. Their particular interest is in how family-based or "familistic" cultures (Banfield (1967))— i.e., cultures with strong family values, including a preference to live close to one's immediate family—can create demand for stringent labor market regulation.

What do they find?

> US [second-generation] immigrants coming from countries with strong family ties tend to consider job security as…important… They are more likely to believe that the government should save jobs or directly intervene to regulate wages.

Alesina and Giuliano (2015) find a similar relationship when making cross-country comparisons: people who live in countries with strong family values are more likely to demand stronger government regulation of the labor market, whether the country itself is rich or poor.

Strong family values appear, on average, to create a demand for labor market regulation, and the alternative, weaker family values—greater individualism—appear to increase demand for laissez-faire labor market policies.

8.2.3 Proxies for Ancestry

How do scholars determine the migrant's nation of ancestry? They use two methods: asking a generic question about where one's ancestors came from or just asking directly where one's mother and father were born. When studies of U.S. populations use the GSS, as Alesina et al. did above, the approach is to draw on a question that asks merely where one's ancestors came from—and respondents are allowed to give up to three answers in order of personal importance. But since second- and third-generation Americans frequently have ancestors who came from multiple countries, and since the regression estimates typically treat the top country of origin as the sole country of origin, scholars are likely underestimating the effect of ancestry.

Consider this analogy: If researchers running a double-blind study of a new medicine's effectiveness mistakenly gave all the test subjects only a half dose instead of the full dose of the medicine, then if we found out that the medicine improved patient health in a small but real way, then we should conclude, probably but not certainly, that a correctly run study that gave all patients a full dose would have yielded an even larger effect. Noise-laden proxies for explanatory variables tend to bias coefficients toward zero in simple cases, as Durbin noted (1954). Thus, proxies that are systematically noisy, like the GSS ancestry proxy, are nearly guaranteed to make any reported effects of ancestry appear artificially small.

However, as mentioned above, some studies of second-generation immigrants do directly ask respondents where their parents were born, a question that few respondents are unlikely to know the answer to. Thus, many studies of second-generation attitude migration have quite precise information about cultural ancestry. Both methods—the noisier question about general ancestry and the more precise question about where one's parents were born—routinely find evidence that attitudes migrate.

8.2.4 Intra-European Attitude Migration[2]

An alternative to the WVS and GSS is the European Values Study (EVS). Borrell-Porta et al. (2017) used the EVS to create a nearly encyclopedic paper that tested out cultural transplant theory by looking at over a dozen survey questions. This team, based at the London School of Economics, looked at attitude persistence among immigrants who moved from one European country to another. In the second generation, ancestral-country attitudes on three questions were very persistent, heavily transplanted from the ancestral country to the new home, with correlations greater than 0.5. Speaking informally, that means that over half of the differences in answers to these questions in the ancestral country appear to be transmitted to second-generation migrants in the new country.[3] Paraphrasing, the questions were these:

If people don't work, do they turn lazy?
Who is responsible for taking care of a person: the individual or the state?
Does a preschool child suffer if the mother works outside the home?

On four more questions, ancestral-country attitudes were moderately persistent into the second generation but showed more fade-out than the above three, with correlations ranging from 0.25 to 0.5, so home-country attitudes predicted about one-fourth to one-half of differences in destination-country attitudes. These cultural transplants were real, but nowhere close to complete. Again paraphrasing:

Do you trust the European Union government?
Do you think most people can be trusted?
How important is family to you?
How important is God to you?

There were quite a few questions where fade-out was essentially complete, where ancestral-country attitudes had little to no bite at all, with correlations between home country average views and the views of second-generation migrants of less than 0.25 (with some even slightly negative). These cultural transplants failed almost completely. A few examples:

Is it bad for women to work outside the home?
Can the police be trusted?
Are you religious?
Are immigrants a strain on the welfare system?

Intra-European migration gives us a chance to test for full migrant assimilation, and at least in the second generation, the outcome is diverse: some attitudes converge to local norms (on average) and some don't. But note that among the most persistent is one question explicitly about the role of the state—whether it should take care of a person—and another is plausibly related to preferences for redistribution and labor market regulation—whether non-workers turn lazy. Trust in the EU is also moderately persistent in the second generation. Politically relevant attitudes clearly can persist among intra-European migrants.

Giavazzi et al. (2019) found broadly similar results when looking at second-generation U.S. immigrants. They separately looked at fourth-generation immigrants. Under a lax measure of assimilation—checking to see what percentage of respondents closed more than half

the opinion gap between first-generation migrants from their country of origin and the U.S. average—they found that when it came to views on cooperation, 81% of fourth-generation migrants assimilated, but when it came to views on government, only 38% assimilated. I should emphasize the laxness of the assimilation measure: closing half the gap by the 4th generation. Even when grading on a curve, it's hard to find evidence of full assimilation. Of course, to the extent that migrants are bringing productivity-boosting opinions and behaviors, that "failure" of assimilation could of course give a great boost to productivity.

8.2.5 Generalized Trust: Attitude Migration and Experimental Correlates

The most-studied question in the attitude migration literature is this one:

> Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?

This question, known as the generalized trust question, has been a staple of social science research for decades, and its national, international, and experimental correlates have been documented and debated in great detail. This short review can of course only touch on this range of literatures; in *Why Trust Matters* (2021), economic theorist Ben Ho offers an excellent nontechnical survey of the trust literature.

At least one empirical finding regarding trust is unambiguous: answers to the generalized trust question migrate from one country to another, and are routinely transmitted to the second generation to a sizable degree. In *The Culture Transplant* I look across a variety of trust migration studies: Canada, Australia, the U.S., and the above-mentioned pan-European study all find that trust migrates and persists to some degree in second-generation migrants. Notably, these results from different countries generally use different datasets, so this is not a one-dataset result. Even among studies that draw upon the widely used WVS, different studies include datasets from differing sets of years. The persistence percentage differs from study to study, country to country, specification to specification, and includes a high value like 71%, and a low value like 17%. I take 40% trust migration as an informal central tendency; a little less than half but a lot more than zero.

Müller et al. (2012), taking a critical approach to trust migration, looked across waves of the WVS to see if the moderate trust migration to the U.S. documented in one influential paper, Algan and Cahuc (2010), was a fragile result that depended on the precise wave (year) of the WVS data. In one set of results, they used a multi-generation adaptation of the measure emphasized here: they included second-generation migrants born between 1910 and 1975, third-generation migrants born after 1935, and fourth-generation migrants born after 1960. Algan and Cahuc called this "Inherited Trust in 2000." Müller et al. compared results across the 2005, 1995, 1990, and 1981 waves of the WVS, in addition to the 2000 survey used by Algan and Cahuc. Algan and Cahuc reported 46% trust migration with this multi-generation measure, and across the four other waves, Müller et al. found three higher rates of trust migration (56%, 52%, 47%) and one lower (33%). The median of these four values is thus 50% and the mean is 47%, a confirmation of the Algan and Cahuc result.

Müller et al. then check two more measures that Algan and Cahuc use, both of which downweight more recent waves of immigrants. One just looks at fourth-generation migrants—the great-grandchildren of the original migrants. Another measure, which Algan and Cahuc called "Inherited Trust in 1935," looks only at second-generation Americans

born *before* 1910, third-generation Americans born *before* 1935, and fourth-generation Americans born *before* 1960. This measure thus puts more weight on the distant past ancestry of survey respondents compared to the Inherited Trust in the 2000 measure; the respondents are also an average of 15 years older in the 1935 Trust sample.

Out of these eight longer-run estimates, three are larger than the aforementioned 46% (72%, 53%, 61%), and five are smaller, with three much smaller: 37%, 29% 12%, 10%, 5%.). These eight estimates are statistically less precise overall—half lack the asterisks that researchers mistakenly (McCloskey and Ziliak (2010)) place excessive weight upon—so the true values could plausibly be much larger or much smaller than the estimated values. But with a median across the eight of 34% and a mean of 33% across these eight estimates, and even higher estimates for the 2000 Trust measure above, there's good reason to treat the Müller et al. study as a confirmation of the attitude migration literature, even if the authors themselves treat it as a critique.

With trust migration well-established by the standards of modern empirical research, one wonders whether such survey responses translate into real-world behavior, as we saw with savings or labor force decisions earlier. Experimental economists have used the famed Berg et al. (1995) trust game to see whether people who say they think people can be trusted are themselves more trusting (investing some experimentally-provided cash to a stranger's project in hopes of getting some money back), more trustworthy (actually returning some discretionary amount of money back to the investor after the project turns a profit) or neither. In a typical game, an "Investor" is handed five one-dollar bills, and chooses whether to keep them all, send them all over to an anonymous partner, or keep some and send the rest to that anonymous partner. The partner—whom I'll call the "Returner," finds that when the money arrives, it has doubled: so $2 sent over becomes $4, for example. The Returner then makes a decision a lot like that of the original investor: does she keep all $4, send back all $4 to the Investor, or keep some and send back some?

So, do trust attitudes predict the behavior of either the Investors or the Returners? In particular, in countries where the WVS reports that people are more trusting, do Investors in this game act in a more trusting manner? Do they send more money over? Johnson and Mislin (2011, 2012) investigated this question. They collected data from trust game experiments run around the world on a total of 23,000 subjects: this led to 152 experimental results from 30 countries that reported the "trust" result (what fraction of the money the Investor sends over). They summarize a key finding in their 2012 paper:

> a one standard deviation increase in the percent of survey respondents who answer 'yes' to the WVS Trust Question is associated with an increase in a country's experimentally measured trust of about 28% of a standard deviation.

They found statistically imprecise and quantitatively much smaller relationships between national trust and experimental trustworthiness (Returner's fraction sent back). The corresponding coefficients, two with negative signs, were one-third or less the size of the trust (Investor) coefficients.

The 2012 paper is, to my knowledge, still the only cross-country comparison of trust game results against WVS trust measures, but it offers a signal of external validity of this simple survey question: and it suggests that in countries with high levels of survey trust, experimental subjects are, on average, more trusting in the lab. Within-country trust game experiments have a more contested set of results, but the heart of the debate is not over

whether the generalized trust question captures anything at all, but whether the trust question captures the respondent's level of *trustworthiness* rather than the respondent's level of trust. Glaeser et al. (2000) famously find that in their Harvard-run trust game,

> [s]tandard attitudinal survey questions about trust predict trustworthy behavior in our experiments much better than they predict trusting behavior.

Notably, the Glaeser et al. "trust predicts trustworthiness" result has been replicated sufficiently—including in rural Bangladesh by Johansson-Stenman et al. (2013), among Peruvian microfinance participants by Karlan (2005), and among Emory University students Capra et al. (2008), that even though the finding is not universal, Rick Wilson (2018) looks at the literature and concludes:

> It would seem that the attitudinal measures [of trust] from the GSS and WVS are correlated, in some manner, with trustworthiness.

One well-known counterexample to these results is Fehr et al. (2003) which instead found in a large sample of the general German population—not just college students—that the survey trust measure predicted trusting behavior by the Investor rather than trustworthy behavior by the Returner.

A literature that dives more deeply into the microfoundations of trust continues to interpret the results of these and other studies. One important possibility is that answers to the generalized trust question may capture *preferences* toward cooperative behavior rather than *beliefs* about the likelihood of trustworthy actions by others. For example, Thöni (2017) investigated another canonical experimental setting—a public goods game. In the public goods game, a small number of players are each given, say, the same five $1 bills. Those players then simultaneously, independently, and anonymously decide how many of those dollar bills to place in a metaphorical pot rather than just keep it; all the money placed in the pot instantly doubles and is then divided equally among all players.

A rational, selfish player would hope that everyone else contributes to the pot, but will choose to "free ride" by putting in nothing. A pure altruist might well contribute everything in order to help the strangers playing the game. In real life, it's common for about 1/3 of the money to be put in the metaphorical pot, though there is wide variation (Ostrom 2000). Based on Thöni's experience in running versions of this public goods game, he concludes that the generalized trust survey question appears to capture a generalized preference for cooperation. As he put it:

> The prominent and widely used TRUST question seems to be a measure for preferences for cooperative behavior, i.e., the willingness to take a leap of faith when entering a social dilemma.

This literature has not settled on a complete explanation for these varied results, and none may be forthcoming. To my knowledge, there's been no systematic effort to integrate the default within-country "trust predicts trustworthiness" result with the Johnson/Mislin cross-country "trust predicts trust" result. But if the answer to this simple survey question usually predicts some form of pro-social behavior in the lab, whether that means more trusting behaviors from the Investor or more trustworthiness from the Returner, then social

scientists should consider a high-trust answer to the WVS trust question to be a statistical signal of higher social capital.

8.2.6 *From Attitudes to Policy to Institutions*

If multi-generational migrants import new (and potentially better) attitudes, will these attitudes change government in important ways? In democracies, can migration shift the political views of citizens sufficiently to lead to broadly based institutional change?

The most direct mechanism for such change would be if elected politicians responded to voters. One important example: Calderon et al. (2023) found that the Great Migration of African-Americans was a notable force in somewhat improving America's institutions surrounding racial equality. When African-Americans moved from the Jim Crow South, where it was nearly impossible for the vast majority of African-Americans to vote, to the North, where it was easier for African-Americans to vote, local politicians in the North became highly responsive to the views of these new voters, and overall pushed harder for civil rights legislation as a result. Indeed, the authors find evidence that white voters in states that had more African-American migrants shifted their political views toward racial equality—so the migrants changed the views of the previous residents. I've referred to this reverse assimilation phenomenon as "The Spaghetti Theory of Cultural Change," since the arrival of Italian-Americans to the U.S. obviously increased the likelihood that other Americans ate Italian food. Assimilation is often a two-way street—where migrants change the views of previous residents and previous residents also change the views of migrants.

Likewise, the scholarly literature on women's suffrage finds strong evidence that U.S. states and nations around the world that gave women the right to vote saw policy changes so widespread that one can only think of them as "institutional" in scope (*inter alia*, Lott and Kenny (1999), Doepke et al. (2012)). Both of these cases—women's suffrage and the Great Migration—are consistent with the predictions of the median voter theorem: When new voters arrive with new views, whether literally in the case of African-Americans moving northward or metaphorically in the case of women arriving to the world of suffrage, politicians who want to win elections will have a strong incentive to move in the direction of those new views. New people create new policies. A second more speculative possibility builds upon Acemoglu's "Why Not a Political Coase Theorem?" (2003) and Olson's Roving and Stationary Bandits model (1993). Even autocratic governments have some incentive to maximize their tax revenue. That means that if a highly productive "market-dominant minority" (Chua (2004)) could become even more productive under better economic institutions, then autocrats have an incentive to improve institutional quality, since such institutional improvements could substantially boost the autocrat's tax revenue. Institutional reform efforts are costly, difficult, and risky, so autocrats and democrats alike are more likely to make the effort when the potential return is highest. And the presence of a market-dominant minority in a country may increase that potential return to reform just enough to spur that reform effort.

8.3 The Deep Roots of Economic Development

Putterman and Weil's (2010) Global Migration Matrix belongs at the beginning of any discussion of the migration-adjusted Deep Roots literature. They estimated which fractions of which populations had moved where since 1500, whether their movements were voluntary or forced. They chose 1500 since, after that time, the age of European migration

and conquest shifted the world's population massively. Where feasible, Putterman and Weil used modern genetic measures to inform their migration estimates. The result of their effort is that economists now have credible estimates of which fraction of each country's population originally hails from China, sub-Saharan Africa, or Western Europe, and so on. This is particularly useful in the New World but is also important when looking across the modern nations of Southeast Asia.

Putterman and Weil also created estimates of the number of millennia (before 1500) that a nation's residents practiced settled agriculture and created a more complicated index that estimates how much ancestral experience living under organized governments a nation's ancestors had. These "Agricultural History" and "State History" scores predict modern income levels, but they do far better when one adjusts for post-1500 migration. For example, the R^2 of the correlation between migration-*unadjusted* Agriculture and State History scores versus modern log GDP per capita are merely 8% and 6%, respectively. However, the R^2 of the correlation between migration-*adjusted* Agriculture and State History scores versus modern log GDP per capita are 22% and 24%, respectively.

One immediately wonders whether this increase in predictive power is merely driven by the "Neo-Europes" mentioned earlier, and so the authors run two-control regressions that include both a simple migration-adjusted measure and a migration-adjusted measure interacted with a dummy for the Neo-Europes. The Neo-Europes have (statistically significant) *negative* signs, a signal that they're underperforming, though the net migration-adjusted Deep Roots coefficient is still positive. A separate two-control horse race between the migration-adjusted measures and fraction of European descent finds positive coefficients on the European descent coefficient but also finds the coefficients of the original migration-adjusted State and Agricultural History measures little-changed from their original values. The "Deep Roots" result that migration has shaped long-run national productivity is thus not overwhelmingly driven by Europe or by the descendants of Europeans. Many people outside of Europe contribute to the statistical explanatory power of the migration-adjusted Deep Roots literature.

These results document clearly that, as many in the literature have put it at one time or another, the history of peoples matters more for the wealth of nations than the history of places. But Putterman and Weil went further, trying out different Deep Roots measures, indexes of the past, to see how they performed when adjusted for migration. One measure they use stands out: Comin, Easterly and Gong's (2010) Technological History index for the year 1500 AD. Comin et al. had a team of researchers dive into reference works in the history of technology in order to create an index of what fraction of the world's technology each nation was using in the year 1500. Their Technological History index outperformed Putterman and Weil's own indices, with migration unadjusted and adjusted R^2 against modern log GDP per capita of 18% and an exceptionally high 53% [see Figure 8.1].

Going beyond the migration-adjusted Deep Roots as predictors of output per person, James T. Ang (2013) made a thorough investigation of whether these three measures—migration-adjusted State, Agricultural, and Tech history (SAT)—could predict modern institutional quality [see Figure 8.2]. Indeed they did, and in all three cases, the migration-adjusted measures outperformed the migration-unadjusted measures. When it comes to institutional quality, the migration-adjusted past is prologue. This isn't just a generic story of persistence, a story that nothing much changes; it's instead a story of *migration-adjusted* persistence, a story that things are quite likely to change in a land when new people move to that land.

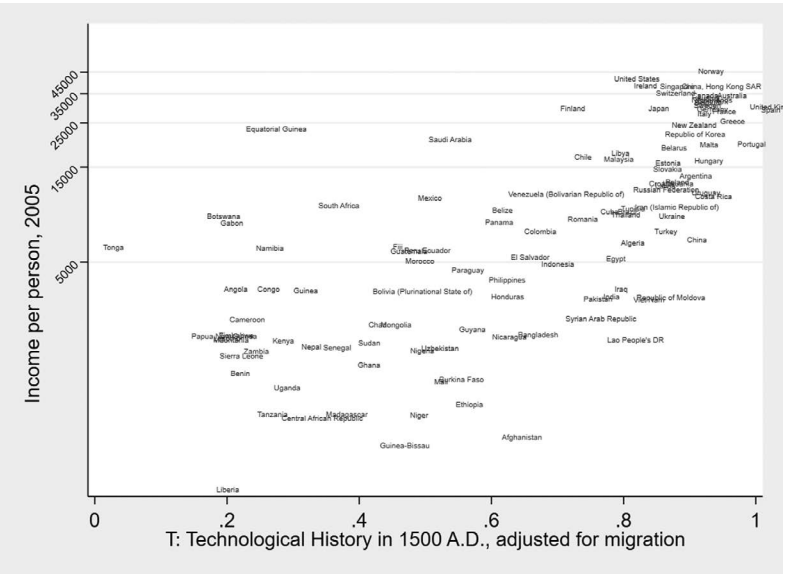

Figure 8.1 Comin, Easterly, Gong (2010) Technological History Score in 1500, Adjusted for Migration vs. GDP per Capita, 2005.

Sources: X-axis is from Comin, Easterly, Gong (2010); the log-scaled Y-axis is from the Penn World Tables. The Pearson correlation is 0.63 in GDP levels; 0.69 when GDP is logged.

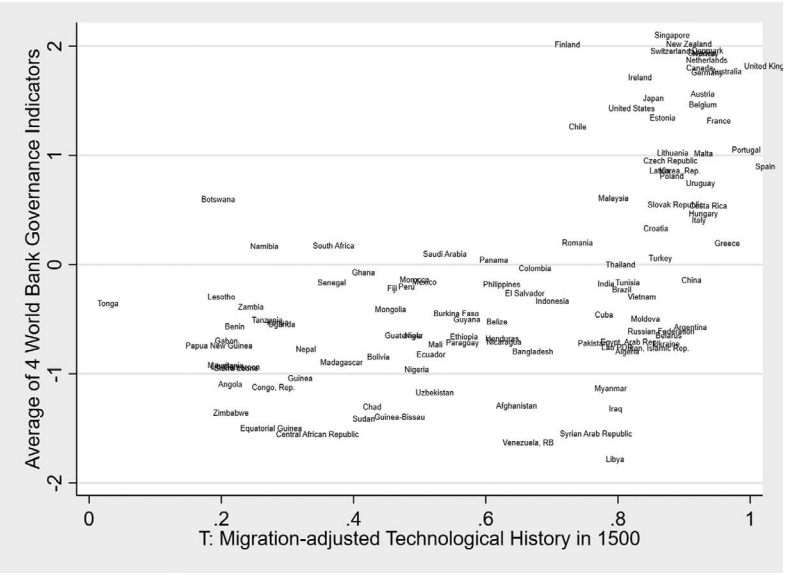

Figure 8.2 Comin, Easterly, Gong (2010) Technological History Score in 1500, Adjusted for Migration vs. World Bank Governance Indicators.

Sources: Comin, Easterly, and Gong (2010); Kaufmann and Kraay (2021). The four averaged governance indicators are Control of Corruption, Rule of Law, Government Effectiveness, and Regulatory Quality. The Pearson correlation is 0.59.

8.4 A Test Case: Southeast Asia[4]

> The Chinese diaspora has … certain unique cultural characteristics that potentially could be conducive to economic growth.
>
> —*Jan Priebe and Robert Rudolf (2015)*

Across the nations of Southeast Asia, there is a strong relationship between the percentage of a nation that is of Chinese ancestry and current GDP per person; given the relatively low Deep Roots scores in the region before the arrival of Chinese migrants, and given the relatively high Deep Roots scores for China in 1500, the Deep Roots theory would predict that, other things equal, a rise in Chinese migration would raise productivity and institutional quality.

The data are generally consistent with that claim. In Malaysia, 33% of the population was reported to be of Chinese descent in a 2002 study by Rauch and Trinidade; 10% in Thailand, 4% in Indonesia, and 2% in the Philippines. GDP per capita in those four countries is $30,000, $19,000, $12,000, and $9,000 respectively. Singapore, vastly richer per capita than these countries, is estimated to have an ethnically Chinese community that makes up 76% of its population. The well-documented "market-dominant minority" phenomenon is clearly visible across Southeast Asia in the Chinese community. Of course, this doesn't mean that all ethnically Chinese residents are rich or even middle-class, but it does mean, in part, that the nation's large, profitable companies are disproportionately run and owned by families of Chinese descent. On average, there is a positive relationship between the Chinese proportion of a Southeast Asian country's population and conventional measures of institutional quality.

Priebe and Rudolf made a more systematic estimate of the economic role of the Chinese diaspora in their 2015 paper, "Does the Chinese Diaspora Speed Up Economic Growth in Host Countries?" Their question is an important one: "We examine whether countries grow faster if their population comprises a larger share of overseas Chinese." After controlling for geography, starting levels of education, and so on, what do they find? A "country's initial [number of] overseas Chinese is positively related to subsequent growth."

Priebe and Rudolf went further and checked to see how economies with large Chinese migrant populations differed from other statistically similar economies: "The presence of overseas Chinese contributes to a country's participation in international trade, [faster growth in machines, equipment, and factories], and increased … productivity, which in turn all stimulate growth." The authors, like many who study Southeast Asia, draw on the evidence for cultural differences as well:

> According to some scholars overseas Chinese are characterized by a comparatively strong emphasis on family economic success, including long working hours, thrifty and dynamic family businesses, and a high emphasis on educating the next generation. These cultural traits, if large enough to leave their footprint on a whole society, might have repercussions for … a country's economy.

So how much do overseas Chinese citizens appear to matter for a nation's economy? Using cross-country data from 1970 to 2010, Priebe and Rudolf find that a 10 percentage point increase in a nation's Chinese share of the population predicts that the nation would grow 0.7% faster per year. Because of compounding, that would mean a nation where

10% of the population had Chinese ancestors would be 10% richer in a decade compared to a similar nation that had zero citizens with Chinese ancestry. And if, hypothetically, these numbers were taken literally, it would mean that China itself should grow 7% faster per year than otherwise similar nations without any Chinese immigrants—countries with the same education level, the fraction that is immigrants, same population density, similar geography, etc. That might sound unrealistically high until you realize that from the death of Mao until 2010, incomes per person in China grew about 8.5% per year according to the World Bank, an astonishingly high rate, especially for such a long period. During that same time, the average income in the typical country grew 2% per year—a 6.5% annual gap—so China's experience isn't that far from what one would predict from Priebe and Rudolf's research.

You might ask yourself whether they found such a strong link because their statistical analysis includes China itself in the average. But it doesn't. The authors intentionally omitted China, Taiwan, Hong Kong, and Macau. This is a study of the Chinese diaspora that includes data from 147 countries but includes only one majority-Chinese country (Singapore), so there's no way for Chinese-majority countries to skew the results. Both casual empiricism and sophisticated cross-country statistical analyses find evidence that the Chinese Diaspora is transplanting the migration-adjusted Deep Roots of prosperity across Southeast Asia.

8.5 Critiques of Culture Transplant Theory

Four prominent writers have critiqued some version of cultural transplant theory: Bryan Caplan (2019; 2022), Alex Nowrasteh (2022), and Clemens and Pritchett (2019). I consider them in turn. Caplan argues that "the biggest flaw" (2019, p. 125) with the Deep Roots literature is that it is ostensibly not robust to population-weighting. Of course, development and growth economists almost never population-weight their samples, in part because economists believe that the actions of governments appear to be central to productivity—nations, not individuals, are the quantitative focus when studying the Wealth of Nations. But consider his statistical critique: Caplan reports that State (S) and Agricultural History (A) Deep Roots scores become small and statistically insignificant predictors of modern GDP per capita when the global sample is population-weighted. That happens largely because the U.S., China, and India are sufficiently big outliers.

But his critique omits Tech History (T), which has long been known to be the most robust of the three SAT scores. Since T proxies 1500 AD technology, while S and A each measure outcomes going back thousands of years, it's unsurprising that T does the best job predicting modern GDP per capita: it's the most recent measure of the three. In any case, when one runs simple population-weighted cross-country regressions of current log GDP per capita on the Tech history measure (T in the SAT score), T is once again a statistically significant predictor of prosperity with a large coefficient (Jones, 2023). Further, running conventional growth regressions rather than level regressions makes all three of S, A, and T statistically significant with sizable coefficients even when population-weighted: China and India may be Deep Roots outliers to some degree, but both are growing quickly, and neither are substantial outliers when it comes to Tech History.

Nowrasteh's critique of cultural transplant theory focuses on a critique of the trust literature. He makes two notable assertions: the first is a claim that the evidence for the multigenerational migration of trust attitudes is fragile, and perhaps an artifact of the WVS wave that Algan and Cahuc used. We've already seen that's not true, and we've seen that the

migration-of-trust result has been replicated with intra-European migration data as well. A second claim is that, as Nowrasteh (2022) alleges:

> [R]esponses to the trust question generally don't predict trusting behavior in real-world micro-level experiments, trust games, or investment games (Algan and Cahuc, 2013…)

Nowrasteh includes many citations beyond this Algan and Cahuc citation, all to support his assertion about that the survey trust question doesn't predict experimental behavior, but that is the first paper he lists. However, consider this quote from the very Algan and Cahuc (2013) essay in *Annual Reviews* that Nowrasteh is referring to:

> But it is worth noting that there is a strong correlation between trust as declared in surveys and levels of cooperation and reciprocity observed in laboratory experiments.

This citation from Nowrasteh thus does not support his claim about the views of Algan and Cahuc. More importantly, as we've seen, the scholarly literature routinely (but not universally) finds an experimental relationship between the survey measure of generalized trust and *either* trusting or trustworthy behavior in experiments, so responses to the trust question appear to be a reasonable proxy for social capital.

Clemens and Pritchett (2019) agree that "[c]ulture and norms have been shown to be transmissible by migrants," and cite key papers in the area. However, they argue that the connection from culture and norms to institutions and productivity is too hazy to build a case for immigration restrictions upon. They do not discuss the Deep Roots evidence for a link between migration and institutional quality (Ang, 2013; Putterman and Weil, 2010), nor do they consider the possible role of the median voter theorem (Caplan 2007) or the lessons one could draw from the Great Migration of African Americans from the South to the North. They thus turn to a model that assumes that there is no permanent effect of migration on institutions or productivity, whether positive or negative.

In their model, they assume that migrants come from a country with a different productivity level than that prevailing in the destination country, and they assume that when migrants arrive, they start off with their old, different productivity level. But Clemens and Pritchett assume full productivity assimilation of those migrants in the long run (at a constant rate *a* per year, $0 < a < 1$). Their assumption stands in contrast to the real-world experiences of, *inter alia*, Chinese migrants across Southeast Asia, who on average have not assimilated to the productivity norms of their fellow citizens in countries like Indonesia, Thailand, and Malaysia. It's also in at least severe tension with James T. Ang's findings that migration-adjusted Deep Roots measures predict institutional quality better than migration-unadjusted measures. Full long-run productivity assimilation is a difficult assumption to maintain in light of existing evidence, and it erases the experience of Chinese migrants. I hope that future work on the effects of migration on productivity can take into account the cross-country, multi-generational persistence of success, on average, among the Chinese diaspora across Southeast Asia.

8.6 Conclusion and Future Work

The cultural transplant literature focuses on the multi-generational effects of migration on the destination country's institutions and productivity. The attitude migration literature finds that savings and labor market behaviors, views on the proper role of government, and views

toward trust all persist to a notable degree until at least the second generation and often beyond. Future work, perhaps using more accurate genetic or archival proxies for ancestry rather than mere survey questions, will, one hopes, be able to more accurately document the blend of cultural persistence and two-way assimilation surely occurs in the real world.

Future work can also document how and to what extent multi-generational migrants, once assimilated into the political systems of their new homes, shape future economic policies. Is the median voter theorem the most important mechanism? Is political representation by the children and grandchildren of migrants an important channel for shaping institutions? Do leaders in non-democracies respond less than leaders of democracies to the migration-induced shifts in citizen preferences? If so, how much less? Do non-citizens or temporary migrants have negligible effects on institutions and total factor productivity in all, most, or some cases? Again, what mechanisms create these outcomes?

Putterman and Weil's 2010 migration matrix can also be updated with the latest information, including more recent genetic data on ancestry. Above all, I hope that the people of the Chinese diaspora, with their example of large-scale, overwhelmingly peaceful migration that appears to have increased total factor productivity and institutional quality across Southeast Asia over the last two centuries, will receive scholarly attention from economists that they deserve.

Cultural transplant theory suggests that there is likely to be a global tradeoff between helping the world's poor by massively opening up migration to rich countries versus helping the world's poor by treating those same rich countries—particularly the highly innovative I-7—as our planet's great research facility. If an I-7 nation chose a migration policy that substantially lowered its Tech History score or its tendencies toward frugality, trust, or trustworthiness, that could jeopardize national productivity in that country. Such a policy could also potentially impose costs on the entire planet for generations to come. As I show in *The Culture Transplant*, a nation's research efforts are highly sensitive to its institutional quality, so any policy reform that reduces that nation's government quality would jeopardize the entire planet's growth rate of scientific and engineering excellence. Further research, of course, is needed to understand how likely such an outcome is and whether skill-based or points-based immigration policies in the I-7 could overcome the risks to long-run global productivity growth documented by cultural transplant research.

It is premature to offer concrete policy recommendations, but consider this proposal: for countries that use points-based migration systems, like Canada, Australia, New Zealand, or Japan, governments could give a small positive weight to applicants from countries where the non-natural resource GDP per capita or national savings rate is higher. This would slightly encourage migration from countries where people tend to have better government quality, higher rates of thrift, and a greater ability to cooperate productively on large, complex tasks. If even a small portion of those traits are imported into the I-7, then those new migrants can offer a gift to their new homelands that may well persist across the generations.

This short essay has necessarily omitted much relevant literature—my omissions of Collier (2013) and Borjas (2015) are especially regrettable—but I close with a quote from Putterman and Weil (2010). Looking across multiple Deep Roots measures of past proxies for productivity, and checking to see how those proxies predict modern GDP per capita, they report their finding, which has been replicated in many forms many times since:

> The most notable finding of the table is that, as expected, adjusting for migration substantially improves the predictive power of any of the alternative measures of early development that we consider.

Past is prologue—a statement that, as Putterman and Weil found, is particularly true when one adjusts for migration.

Notes

1 Since this attempt to hold "other things equal" in empirical comparisons is so common in empirical research and so difficult to do properly, let's discuss this particular example: when comparing across survey respondents who had the same age and gender as each other, those whose parents came from moderately thriftier countries typically saved 1.6 percentage points more of their income compared to those whose parents came from less thrifty countries. But if instead of making that simple comparison, one instead compared people with not only the same age and gender, but also those with similar incomes, similar employment status, similar education levels, and so on, then those with parents from thriftier countries typically saved only 0.8 percentage points more of their income.

If we wanted to know whether second generation migrants from thrifty countries are higher savers, which is the better comparison? The first estimate, the 1.6 percentage point effect. Why? In part, because it's comparing two features that are extremely unlikely to be affected, either directly or indirectly, by the level of thriftiness in the country of origin—just age and gender. The second, bulkier comparison looks at savings differences between people who are also similar in income, employment, and education—traits that are themselves plausibly influenced by the thriftiness of the culture one's grandparents were raised in.

We want to know what "the typical migrant whose parents came from France" is like, not what "the typical migrant whose parents came from France and who earns just as much as the average immigrant whose parents came from the USA" is like. That extra "control," that extra attempt to "hold other things equal" ends up bringing a quite unrepresentative hypothetical person into our comparison, and that hypothetical person probably has a different and quite uninteresting savings rate than the first person we were looking at.

There is a broader lesson here: this problem of possible overcontrol bias [trying to hold the "wrong things equal"] in multivariate regressions is real, and it's important not to presume that more controls are better, as Julia Rohrer (2018) has recently emphasized.

2 This subsection draws language verbatim from *The Culture Transplant* (2022).

3 Correlation coefficients are the square root of the R^2 of a bivariate regression as well as the regression coefficient from *any* bivariate regression where both left- and right-hand side variables have been transformed to have mean zero and standard deviation of unity. Since R^2 is the "percent of variance explained," and variance is itself a sum of *squared* differences across the observations on the left-hand side, then it is both natural and wise to interpret the correlation coefficient, R, as the percent of differences in the Y (left-hand side) variable that can be predicted by the X (right-hand side) variable. For estimates that aren't driven by outliers, R is usually quite close to the sum of absolute deviations in the Y variable that can be predicted solely by deviations in the X variable.

4 This subsection draws language directly from *The Culture Transplant* (2022).

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